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# *Interactive comment on* "Sea-air $CO_2$ fluxes in the Southern Ocean for the period 1990–2009" *by* A. Lenton et al.

**Anonymous Referee #2**

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This is a very well written and well presented manuscript. The overview of the Southern Ocean carbon cycle, through a consideration of a variety of approaches, is a long-needed synthesis of state-of-the-art carbon budgets. This contribution makes a valuable contribution to the RECCAP effort, will contribute to both Southern Ocean research as well as to broader efforts to constrain the global carbon cycle. I suggest below a few suggestions for how to strengthen the scientific content of the paper, and assuming that these suggestions are addressed I think that the paper should be suitable for publication.

The first point is bibliographical, and relates to the forward biogeochemical models. In discussing the similarities and differences between the various models, the authors should state clearly whether the models are using Geider (1997) or another represen-

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tation of growth rates. There is growing awareness that aspects of the Geider model, in particular the temperature dependence of growth rates, may lead to biases in simulating the seasonal cycle of biogeochemistry. The authors clearly are not responsible in this study for identifying the reason for the bias in seasonality, but it would be very helpful to have a brief discussion of this matter. Can the authors quantify the systematic uncertainty (disagreement between models) and relate this to the presumed amplitude of the target signal for the decadal trend in air-sea fluxes over the Southern Ocean?

Second, and again with respect to forward ocean biogeochemical models and the seasonal cycle, it would be appropriate in the discussion on page 304 (lines 13-25) to mention that water mass transformations are likely to play a first-order role in determining the preformed contemporary carbon concentrations over the Southern Ocean, as has been demonstrated for the natural carbon cycle by Iudicone et al. (2011; BG). I'm in fact a bit surprised that the Ocean Inversions and the Forward Models are so similar in their large-scale uptake estimates (the first part of Fig. 3), since for Forward Models one is considering fluxes over the region to the South of 45S, whereas for Ocean Inversions one is considering uptake for isopycnals that outcrop in the region to the south of 45S. So if Talley et al. (2003) and MacNeil et al. are right, and more than half of the formation source of SAMW is surface subtropical thermocline water, the results with the Forward Models and the Ocean Inversions could have been somewhat divergent, as they in fact may be considering different carbon quantities (Air-sea gas exchange for forward models and subduction fluxes between the mixed layer and interior for ocean inversion models). The authors should clarify this point if I am not mistaken here in my interpretation of the ocean inversion models. I think that referring to Figure 2 of Iudicone et al. (2011) is a good point of reference here for the general point here about water masses and carbon.

Third, the time series shown in Figure 10b is strikingly disconcerting. It seems to clearly illustrate that the systematic uncertainty, taken as the inter-model spread, is at least as large as the target signal over the timescale of a decade. It is very important that the authors quantify this systematic uncertainty, and state very clearly in the text this uncertainty. I think that the final sentence in the abstract is misleading in this respect, it would be much better scientifically to be very clear in stating that "...resolving long term trends with atmospheric inversions is difficult due to the fact that systematic uncertainty in this method is of the same order as (or larger than) the target signal on decadal timescales" The problem is not the decadal timescale of this study, in order to be acceptable for publication it is very important to be clear and transparent about this serious point, and to state up front (quantitatively) the challenges the atmospheric inversion method faces. Within the Discussion section, it would also be appropriate to review candidate methods for improving this serious problem. Can either the join inverse method of Jacobson et al. or the inclusion of c13 as proposed by Rayner et al. help to reduce the systematic uncertainty, in particular over the Southern Ocean? If this has been discussed in the published literature, it should be mentioned here. Over what timescales do the authors believe that the systematic uncertainty of the atmospheric inversion method will be less than the target signal? It would be beneficial for the authors to quantify this in the text as well. For context, are there other regions or ocean basins where the systematic uncertainty with this method is less than the target signal on decadal timescales, in other words, where the method seems to work? Surely atmospheric inversions will continue to play an important role in climate research, but a quantified view of the challenges this method faces must be included in the text.

Once these points are clarified, I believe that this paper will be of broad interest to the climate science community, and that it should be ready for publication.

Interactive comment on Biogeosciences Discuss., 10, 285, 2013.